

POLICY AS A MOVING TARGET:
A CALL FOR CONCEPTUAL REALISM

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ABSTRACT

Policy as a Moving Target

Since the early 1970s, policy scholars have paid great attention to issues of policy implementation, treating it as the "missing link" between policy formulation and results. Most often, policy problems have been seen as static phenomena that can be corrected by carefully specified programs. This paper, drawing upon examples from Swedish energy policy, argues that this is not the case, that political and social conditions are so prone to change that implementation must be a dynamic process if it is to be effective. This has distinct conceptual and practical implications for the study and design of policy implementation.

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I. INTRODUCTION

Much has been written in the past few years regarding the "coming of age" of the policy sciences or, at the very least, public policy analysis. Virtually every major American and many a European university has a program in the art, if not the discipline. A number of journals define the field and its materials; an encyclopedia of policy studies has been assembled to encompass the subject (Nagel [ed.], 1983). While few would doubt the professional burgeoning of the field, many qualified observers have wondered whether this rapid expansion has not cavalierly left behind the intellectual and normative requirements its early advocates urged. This is not to suggest that public policy researchers are consciously callow but rather that the issues being addressed are remarkably resistant to easy analysis, that the methodological and subsequent policy shortcomings experienced are more the fault of the subject matter than the practitioner. When a "science" is more characterized by "social interaction" than "intellectual cogitation," (Wildavsky, 1979b), then one should expect to see either a certain amount of empirical laxness or oversimplifying behavioral assumptions. Policy analysis has been accused of such epistemological negligence, thus calling into question its very *raison d'etre* (e.g., Schneider et al., 1982).

The validity of such charges is certainly open to argument (cf. Schneider et al., 1982, Brunner, 1982, and Tornatzky et al., 1982). Still, it is safe to say that as policy researchers have become more practiced and sophisticated in the policy environs, they have become increasingly aware of the complexity of policy issues. Combined with the tide of legitimate sponsor skepticism concerning policy research (Rein and White, 1979), policy research has been forced to retrench intellectually, to bring under harsh scrutiny its very approaches to policy problems, their underlying assumptions, and the resulting policy recommendations.

This paper directly addresses one of the simplifying assumptions that has too often been implicitly a part of policy research, namely, that policy can be treated as static and policy processes as stable. As part of its economic and public administration heritages, for purposes of analysis, *ceterus paribus* has been a working rule of thumb; analysts were simply unable to treat a world in which multiple variables were permitted to change, sometimes independently, occasionally in unison. Large-scale computer simulations could computationally accommodate elaborate simultaneous equations, but the underlying theory and supporting data were generally so suspect as to render their results at least problematic if not actually erroneous (Ascher, 1978). For instance, policy evaluation researchers were beset with the problem of assessing programs that were designed to operate in a specific context towards given goals (if they were even fortunate enough to have these goals defined) at a time when the contexts and very possibly the objectives were changing (Cronbach, 1980). In some instances, a Heisenberg effect prevailed; the very success of a set of programs made rigorous evaluation extremely difficult because it effectively altered the policy context. Whatever is said about the policy sciences and policy analysis, one can confidently say that they have not mellowed with age, that the expansion of the field has not made life any easier; indeed, it is fair to claim that they have grown more difficult as their proponents have been forced to abandon the working assumptions that had smugly underlaid so much of the discipline and profession.

The transient, occasionally turbulent nature of policy issues has been noted in terms of their normative underpinning (Kaplan, 1963) and practical applications (Lasswell, 1971; more recently, Brewer and deLeon, 1983). As noted above, this problem is quite apparent in policy evaluation. This essay examines another area of policy research in which the mutability problem is critical, policy implementation. We think implementation is particularly critical in this analysis for two reasons. First, as many researchers (Hargrove, 1975; Williams, 1975; Bardach, 1977) have argued, policy implementation is the crucial nexus between policy formulation and policy effect; without faithful policy implementation, there can be little congruence or fidelity between plans and results. If this is true, then to demonstrate that policy implementation is at odds with political realities (i.e., that it is based upon a static environment) is to uncover a serious flaw in policy research which calls for swift remedy. Second, implementation is an area of policymaking which seems most alluringly amenable to formal planning, or what Berman (1980) calls "programmed implementation" and Majone and Wildavsky (1979) term "planning and control model of implementation." While the assumption might be intellectually convenient, it is predicated on the unrealistic assumption of being able to predict future key variables and events. Surely this is a highly dubious assumption, again one that demands correction.

This paper thus considers one problem that the policy sciences must address if they are to grow in such a manner that they can analyze public policy problems characterized by increasing complexity--namely, the changing contextuality in which the problems exist, that is, the variant nature of the problem itself as it is treated in the policymaking process. We shall examine this issue in terms of policy implementation, which we explicitly assume to be representative of similar problems in other stages of the policy process. Our examples will primarily be drawn from a history of Swedish energy policy. We present these examples because we are confident that they are representative of a larger universe of implementation issues and they are sufficiently interesting as to warrant wider exposure. Finally, we will offer some suggestions as to what one might do to remedy the dilemmas we have identified.

II. IMPLEMENTATION ANALYSIS

Since its initial recognition in the early 1970s, implementation research has gone through an intellectual life cycle which seems uncomfortably familiar. Early enthusiasms and pioneering efforts blazed a trail to fields of expanding research activities. Characterized as "the missing link" in policy activities (Hargrove, 1975), genuinely original and innovative scholarship initially thrived in terms of both case studies (Pressman and Wildavsky, 1973), typologies (Bardach, 1977), and analytic constructs (Rein and Rabinovitz, 1978). These were soon replaced by a more repetitious reliance on relatively commonplace case studies which were optimistically proposed to serve as a part of departure for later, more comprehensive studies (e.g., Mazmanian and Sabatier [eds.], 1981). This is not to suggest that there were no exceptions (see Rabinovitz, Pressman, and Rein [eds.], 1976) or that these case studies were unimportant. But, as one observer wrote at the time, implementation research seemed destined to become more faddish than useful:

Everybody seems to be studying it if not solving its problems.... In the rush to get on the bandwagon, studies of bureaucratic politics, organizational analyses of resistance to change, analyses of policymaking and decisionmaking, and cases in public administration have a new focus-- "implementation analysis." (Berman, 1978: 158)

We do suggest, however, that such studies did not lead to a significant advancement in understanding the problems of implementation. In place of an emerging theoretical consensus or practical application, debates started to form around otherwise innocuous points of controversy which were made embarrassingly visible through such conspicuous tags as "up/down" "inter/intra," "micro/macro," and "formal/informal." What was clearly lacking was an integrating, underlying coherent theory. This shortcoming is hardly trivial. As Hargrove (1980: 280) has cogently noted, "Policy analysis that lacks theoretical underpinning is incomplete. Theory that is not tested through application remains academic."

Again, we wish to stress that these efforts were not wasted nor counterproductive. Important definitional issues were tackled, if not resolved (Bardach, 1980) and several propositions were extracted which deduced how "successful" implementations might have been achieved (Bowen, 1982; Sabatier and Mazmanian, 1983). Few seemed willing, however, to examine the central issues which underlie a theory of implementation, that is, what made implementation work or not work. Others argued that perhaps we really could not understand the answer to these sorts of questions, for implementation was basically the political act itself, always present and ever illusive. Wildavsky commented that "Implementation is no longer solely about getting what you once wanted but what you have since learned to prefer until, of course, you change your mind again." (Wildavsky, 1979a: 176) In short, implementation researchers were extremely busy but their cumulative results did not seem commensurate with their profligate activities.

Therefore, rather than directly confront basic implementation theory, policy researchers began to talk about more practical matters, such as "implementation analysis," which Williams (1975: 558) defined as investigating "(1) the technical capacity to implement, (2) political feasibility, and (3) the technical and political strategies for implementation." Bardach (1980: 156) less kindly characterized this research vein as "an exercise in concentrated pessimism" while Wolf described the newer and more circumspect goals of implementation analysis in systems analysis terminology:

Implementation analysis, as a regular component of policy analysis, should link the formal modeling and cost-effectiveness comparisons among alternative policies with consideration of how policies are likely to be altered if implemented. It is intended to explain and anticipate the frequent tendency of implemented policies to result in higher costs and lower benefits, as well as different consequences, from those calculated in conventional policy studies (1979: 139).

Implementation analysis was directed towards the applications aspects of implementation research (Elmore, 1979-80). It owed more to policy analysis' systems and economic analytic heritages than its political lineage. It heroically held to the assumption that implementation strategies could be confidently programmed or planned in advance. Otherwise, why would one engage in the implementation analysis exercise in the first place? Writes Wolf: "Addressing the questions in specific policy contexts requires that they be reformulated with precise reference to those contexts. For each policy alternative, the cardinal implementation issues ('who has to do what, when, and how?') cannot be avoided." (Wolf, 1979: 139; cf. Lasswell, 1958)

We now seem to face a situation where implementation research is thriving but where it might also run the risk of becoming the victim of its own success. True enough, most of the original societal impetus for this type of inquiry is still there. Even in an era of large budgetary deficits and slow growth economies, public programs are as big as ever, and policymakers face as substantial a set of problems now as they did ten years ago if they are to translate policy commitments and societal aspirations into real world effects. Of course, there is wide-spread disillusionment after a decade and a half of extensive exercises in knowledge utilization for public policy (Lindblom and Cohen, 1979). But little or nothing suggests that politicians should be less prone to draw upon whatever policy relevant studies they might feel are of use to them in political and administrative battles or that social scientist should now be more inclined than in the early 1970s to withdraw into academic ivory towers. Thus, there is no realistic reason to expect that interest in implementation analysis is about to or even should wane.

Yet, by virtue of its very vitality, implementation research has tended to expand until it is used as a catch-all phrase. We can also witness its diffusion in terms of the growth of subdisciplines under the umbrella subdiscipline, namely a growing body of overviews, sometimes enlightening analyses, sometimes little more than compilations of the field of of implementation research. Scholars are asked to encompass both institutional and interorganizational perspectives, as

well as be analytic and policy relevant. This irregular, inconsistent growth requires that we take careful stock of some major issues in implementation studies without getting bogged down in purely terminological issues.

For instance, implementation analysis has to a large extent revolved around an on-going controversy between advocates of two different analytical perspectives conveniently identified as "top-down" and "bottom-up." The *top-down* approach originally started out from a basically hierarchical and rationalistic public administration conception of the policy process. It tended to stress the need for a careful delineation of policy objectives into manageable and clearly defined elements, unambiguously assigned to administrative units which guaranteed the smooth passing on of public policy intentions into public policy effects. The *bottom-up* perspective, on the other hand, emphasized the discretion of the "street level bureaucrats" (Lipsky, 1971 and 1978). It asserted that the factors which determine policy impact are outside the sphere of direct government control and have to do with transactions between public programs and environments often dominated by private markets. Thus, implementation analysis must *per force* include an understanding of the structure of linkages between public and private sectors in the different "bargaining arenas." It also has to focus on those public institutions which are most proximate to crucial transactions and then trace the real workings of an implementation process from this bottom level upwards (e.g., Elmore, 1980, and Berman, 1980).

The bottom-up perspective tended to be reinforced by interorganizational analysis (e.g., Hanf and Scharpf, 1978). This union highlighted the methodological problem of delineating proper units of analysis. The idea of an implementation structure as a new type of unit of analysis was developed as a response to this challenge by researchers at the International Institute of Management (Hjern and Porter, 1981). Such structures were defined in terms of institutional commitments to a program rationale. Consequently, they did not coincide with given administrative and organizational hierarchies but rather had the character of self-selected clusters of parts of organizations--some public, some private, and almost always, their interactions.

Furthermore, there is no consensus between either perspective as to the analytic problems of ascribing action-like properties to structures, networks, or subsystems. Some scholars view this just as a convenient shorthand way of describing social reality. Others argue that it involves an illegitimate imputation of capacities of strategic deliberation to entities which simply do not possess such capacities. Both claim that the other distorts an accurate depiction of the implementation process and its mechanisms.

There can be little doubt that the controversy between a top-down versus a bottom-up implementation perspective has stimulated policy research in the field. Today, not surprisingly, we are witnessing a convergence of views. Both schools appear willing to view the different perspectives as complementary rather than mutually exclusive. Richard Elmore's term "reversible logic" neatly captures the idea of a matching of the "forward" and "backward" mapping phases of policy analysis (Elmore, 1979-80 and 1985). Members of both traditions seem prepared to grant that some types of informal networks or policy subsystems are of crucial importance and that these subsystems or networks cannot be viewed in total isolation from formal administrative and organizational apparatuses (see Halperin, 1974; Hanf and Toonen, 1985). The very concept of an implementation structure rests implicitly on an assumption that such, often hierarchical, organizations exist and serve as pools from which elements of the structures are drawn. If everything were just interaction among informal conglomerates--if implementation were completely interactive, to adopt Wildavsky's and Majone's term--then the idea of an implementation structure becomes highly elusive.

Thus, in some respects, the reputed distinction between a bottom-up and a top-down perspective is not clear-cut. A certain convergence of views can be seen around some of the putative points of controversy. This should not give rise to a sense of complacency among implementation researchers. Instead, it should be viewed as an opportunity to re-examine some of the fundamental theoretical issues which are implicit in much implementation analysis but rarely are faced.

The relatively new focus by policy researchers on implementation analysis as opposed to implementation theory is therefore probably well-warranted. The idiosyncrasies of specific program implementations are seemingly not ready for synthesizing into a broad-reaching theory. Hence, the emphasis on particular implementation analyses, mid-range theory, and typologies might be completely appropriate. But we would like to move one step beyond this respite and suggest that part of the problem with implementation to date rests with the underlying assumptions rather than the disparate state of the data or evidence. The convenient assumption that implementation can be viewed against the background of a static set of circumstances upon which programs can be imposed, however welcomed to the theoretically-oriented scholar, fundamentally misconstrues the realities of implementation and, as a result, inhibits the formulation of a sound theoretic basis.

This characterization of implementation analysis as a homeostatic or rigid approach should not be treated as universal. Berman (1980) and Majone and Wildavsky (1979) propose "adaptive" and "evolutionary" implementation strategies, which suggest that policy implementation strategies must be contextually designed and take into account changing conditions. But they do not confront the problems this advice entails when moving beyond the particular and trying to formulate overarching conceptual frameworks. While this paper does not pretend to such holistic grandeurs, it does deliberately attempt to rise above individual case studies and their aggregation and to propose some fundamental observations which should be considered when the theoretical symphony of implementation theory is being scored.

III. CONTEXTUALITY AND POLICY ANALYSIS

Policy analysis not only seeks to understand and explain various courses of actions and their effects. It also is concerned with policy design and policy improvement (cf., e.g., Barrett and Fudge, 1981: 6, with Benson, 1984) or, in Lasswellian terms, knowledge *of* and *in* the policy process (Lasswell, 1971). Thus, some version of an actor-oriented social choice perspective is central to policy analysis. Such a general observation is compatible with fairly different

interpretations of the proper role of policy-oriented scholarship in which three broad traditions can be identified.

First, policy analysis has sometimes been presented as basically a *management science*, characterized within this strand by an ambition to take a relatively broad look at problems of political feasibility and at the range of available alternatives (Quade, 1970 and 1975). Second, policy analysis has been assigned a more grandiose role as "a new supradiscipline" which is more "concerned with the contributions of systematic knowledge, structured rationality and organized creativity to better policymaking" (Dror, 1971: ix). What is required in this perspective is a *science of meta-control* which will supply "knowledge about the direction of the controls themselves, that is, about meta-control" (Dror, 1979: 284). Policy analysis should then not only be concerned with minor tinkering to achieve improved efficiency in the delivery of public services. It should rather immodestly lay the foundations for "governance redesign for handling the future" (Dror, 1983: 14-32). Whether this involves risks for bureaucratic hierarchy is less relevant to the proponents of this tradition than the fact that in the absence of this kind of policy analysis, Western governments are seen to be vitally threatened by "capacity deficits" on such a scale that their future viability itself might be endangered.

But, third, when Harold Lasswell originally outlined the field of the policy sciences, he spelled out a program which intentionally transcended the managerial and control perspectives. This third tradition emphasizes the role of policy analysis as *problem-oriented scholarship* (Lasswell, 1951 and 1971). This problem-orientation demanded a focus on "the basic conflicts in our civilization...the fundamental problems of man in society, rather than upon the topical issues of the moment" (Lasswell, 1951: 8). It required that the analysis was open to the contribution of a diversity of methods and that it was based on a contextual understanding of the larger social setting of the events studied as well as of their evolution over time. Hence, the policy-oriented scholar must consciously "cultivate the practice of thinking of the past and of the future as parts of one context." Although this challenging legacy is still very much alive, it still largely waits to be carried out (e.g., cf. Brewer and deLeon, 1983;

deLeon, 1981; Brunner, 1984; Wittrock, 1983a). It is with this third tradition that we identify and propose as a means to alleviate some of the problems of implementation research.

If policy scientists are to take the contextuality and problem-orientation requirements seriously, then it is necessary to connect studies of policy processes to political and societal macro-analyses. Admittedly, such a project faces formidable obstacles (Mayntz, 1982: 79). But the pervasive and encompassing nature of contemporary policy problems might well demand such an intellectually arduous effort if the complexities of current public policy problems are to be effectively met. The immediate vehicle for such an examination is a discussion of three aspects of implementation analysis which is derived from the contextual nature of the policy sciences in general and implementation research in particular. These are the issues of (1) actors and structures; (2) duration and discontinuity; and (3) systemic asymmetries as they occur in policy evolution. Although they are not mutually exclusive, let us examine each in turn.

Actors and Structures

Policy analysis, including interorganizational implementation analysis, has always been concerned with policy options and strategic choice. This certainly does not preclude an awareness of elements of the institutional underpinning of policymaking, whether in the guise of constitutional rules or a more or less class-based system of corporatist or pluralist representation and mediation (cf. Burns, 1984; Scharpf, 1983). However, efforts to combine an actor-oriented perspective with a structural one characteristically confront very substantial difficulties. In particular, the border line and relationships between actors and institutional environments, between strategic choice and strategic constraints, are rarely, if ever, clearly spelled out (Giddens, 1979: 49-95). This problem is only exacerbated as the policy space becomes more populated (Richardson, 1982).

The way in which this difficulty surfaces depends largely on the "metatheoretical framework" (Kiser and Ostrom, 1982) of the given inquiry. If the framework is microinstitutional and individualistic, it will seek to trace back all assumptions about collective actors to

statements about the behavior and properties of individual human beings. Such an approach tends to allow for accounts of the role of actors and that significant class of institutional and systemic constraints which can be unambiguously derived from statements about individuals, such as cognitive limitations. However, many social scientists, not necessarily limited to political economists steeped in a Marxian and macroinstitutional tradition, would argue that important structural conditions of human action are thereby unduly neglected (e.g., Benson, 1982 and 1984). Furthermore, many policy scholars hold that there are no compelling reasons for analysts to commit themselves deeply to any particular discussion of methodological individualism with its different means and implications. They would rather concur with the more agnostic position of a recent overview:

Organizations do not have brains, but they have cognitive systems and memories. As individuals develop their personalities, personal habits, and beliefs over time, organizations develop world views and ideologies. Members come and go, and leadership changes, but organizations' memories preserve certain behaviors, mental maps, norms, and values over time. (Hedberg, 1981: 6)

But if this kind of agnostic stance is granted, what about the counter-claim that abandoning strict individualism paves the way for "theoretical models with social forces beyond the influence of individuals" (Kiser and Ostrom, 1982: 183)? Obviously this spectre cannot be lightly exorcised. (See Scharpf's point about the limitations of a corporatist perspective; Scharpf, 1983.) Conversely, there appears to be neither empirical nor theoretical grounds for believing that this risk could not be avoided (cf. Olsen, 1981; Offe, 1983; Wollman, 1983). In either case, one needs to admit that the relevant implementation actors and structures may vary throughout the process (e.g., deLeon, 1979), and only occasionally do so with any confident degree of regularity or predictability.

To assess claims like these and also to take seriously the notion of temporal contextuality inherent in the legacy of policy analysis as a problem-oriented scholarship, a long-range perspective on policy evolution should be adopted, and some assumptions about choice and

constraints in the shaping of "the flow of events in time" (Lasswell, 1951) explicitly spelled out.

Interestingly enough, this conclusion appears to be equally valid given the claims of those implementation researchers who argue that when we come across seemingly "variable and inconsistent behaviour this is not necessarily implementation failure but just a reflection of the value dissensus [*sic.*] in which the policy was born" (Barrett and Hill, 1984: 223). If, furthermore, "policy" is defined as nothing more or less than "something that one group of actors wishes to be carried out by others" (ibid: 222), then some firm notion of temporal and societal contextuality is needed if implementation is to retain any meaning above and beyond that of an ever-present and ever-changing seamless web of social interactions.

Duration and Discontinuities

Much of the impetus behind implementation analysis evolved out of a reaction against a simplistic hierarchical account of the policy process. Policies were centrally and unambiguously decided and then passed on for faithful execution on the local level. This idea, if ever really believed, has been firmly disabused. Few implementation researchers, however, were prepared to go all the ways towards embracing an anarchic conception of the policy process. Perhaps the "radical rationalism" proposed by early systems and policy analysis did not hold, but surely there is some forceful logic to being able to design the implementation of the objectives and plans inherent in policies with some fidelity. The search for implementation structures defined in terms of a commitment to a program rationale constituted one candidate for a solution of the problem of rationality beyond hierarchy. Still, once the time perspective becomes sufficiently extended, it is quite possible in some policy areas to find informal networks which are active both before and during the implementation of some particular program. The top-down versus the bottom-up controversy thus reappears, this time in a temporal garb.

In terms of Swedish energy policy, during this entire century, informal networks of researchers, technicians, and groups in industry as well as in public administration have played a key role in maintaining

national interest and competence (Wittrock, Lindstrom, and Zetterberg, 1984). On several occasions, the existence of these networks have been crucial in the drafting, launching, and swift implementation of public programs for energy and energy research and development (R&D) policies. Yet, coherent programs have not emerged through processes of smooth and consensus-like evolution or as a result of gradually converging policy theories of different advocacy coalitions (Sabatier and Hanf, 1985). Instead, in this field, we can witness an instructive interplay between actors' perceptions and policies and structural characteristics and conditions of policymaking and policy implementation, even though the national objectives remained relatively constant.

Swedish energy policy throughout the twentieth century has been based on a surprisingly consistent perception of the nation's energy problem and set of national objectives. Time and again efforts have been made to promote energy technologies which reduce dependence on foreign and finite energy sources and to develop a more secure supply from domestic sources. A bill presented at the 1900 parliamentary session succinctly summarized the situation: "The whole independence and economy of the nation has by virtue of this fact constantly a sword of Damocles hanging over itself." Removing this sword has been the consistent thrust and parry of Swedish energy policy since at least the 1890s. For instance, ambitious efforts were made to promote peat and hydroelectric power--"white coal," the "patriotic fuel"--around the turn of the century, actions well preceding the renewable energy emphases of the 1970s. At the turn of the century, growing imports of coal were seen as an immediate source of difficulty; in the 1950s and 1970s, oil imports played a similar uncomfortable role

Some of these efforts have met with success, most notably in the case of the breakthrough of hydroelectric power, where pioneering technological developments occurred in the wake of a socio-political battle, essentially between the old agro-conservative forces and an alliance between the political left and the new industrial interests, extending over several decades (Lundgren, 1980 and 1982; Wittrock and Lindstrom, 1984). This process involved deep-seated changes in societal organization, including definition of property rights, and also affected concepts of economic feasibility on an energy market. In the case of

most other energy developments, such as those for the production of peat, no similar developments occurred; despite quite wide-spread political support, most of these efforts proved abortive.

In the era after the Second World War, Swedish energy policy programs on a much larger scale have been initiated, but their basic objective has remained quite similar. This is well-illustrated for the government's 1956 decision to establish a major technology program to develop and construct domestic heavy water nuclear power reactors as a means to reduce increasing imports of oil and to keep the entire nuclear fuel cycle within the country. This program--the so-called "Swedish line"--was technologically very ambitious and actually eased the way for a build-up of technical competence when Swedish light water reactors were later introduced and built. However, like so many other programs, the Swedish line was created under the impression of a crisis situation, in this case caused both by rapidly rising imports of oil and the release of large amounts of previously classified material about nuclear technology in connection with the 1955 Geneva conference on the peaceful uses of atomic energy. The program also envisaged far-reaching government direction and intervention to secure the success of the program. Although no formal state monopoly in the nuclear field was suggested, there was no doubt that the government intended to take on the main responsibility for the development of the technology and for overseeing all non-state activities.

The way in which this large program gradually disintegrated and succumbed to the combined onslaught of available American as well as domestic light water reactors, a period in which the policy objectives defined during the crisis condition eroded, is as good a case as any of what Peter Hall terms a "great planning disaster" (Hall, 1980). But the disaster was not caused by simple miscalculation, bureaucratic sabotage, or by a process lacking in broad participation; reports were widely circulated to all interested parties for comment. In fact, one of the problems the program faced was not its lack of participation but its excessive efforts to include fairly divergent public and private interests into one and the same development coalition. The problem was fundamentally one of significantly altered contexts and priorities. In several nations, newly-emerging conditions, such as the recognition of

the harmful effects of "acid rain" on the environment or the delays instituted by litigation, have created added, unforeseen problems with programmed implementation in the field of energy policy. In the Swedish context, the protracted nuclear versus renewable energy debate (Monnroth et al., 1981) has reflected the continuation of objectives and implementation disputes of a long-standing nature.

In light of these features of the political and social landscapes, Hall's prescriptions for remedying and avoiding planning disasters appear to be little better than simplistic. His calls for a still further look into the future by forecasters and for a still greater participative effort to draw on all conceivable opinions in the planning process seem, upon reflection, naive. What he fails to face and what so much of planning and implementation analysis disregards is the fact that there are significant structural and social asymmetries between conditions of policymaking in periods of crisis and in more normal periods where established market-like forces are assumed to operate as usual. The longer the time frame of a policy, the more likely these problems will be encountered and invalidate the implementation strategies. Marvel's "winged chariot" offers no free ride to the implementation analyst, who certainly does not enjoy "world enough and time."

Discontinuities in policy evolution are then not just accidental or random. They occur not just because policies have been erroneously conceived or because anarchic implementation processes can derail even the best planned policy. They also occur because structural conditions do not remain stable over the duration of the problem or the program (Gibbon, 1984). As Disraeli cautioned more than a century ago, "Change is inevitable. In a progressive country, change is constant." Some of these changes are systemic in nature and can neither be treated-- as suggested by Hall--through a more thoroughly pre-programmed policy process, nor just by way of allowing for an ever-widening degree of discretion (or "interaction") in the implementation process (see Offe, 1984, for one perspective).

Systemic Asymmetries and Policy Drift

Policies "travel," we are warned. They "are continuously transformed by implementing actions that simultaneously alter resources and objectives" (Majone and Wildavsky, 1979: 184). Most policies have multiple objectives and even move across different policy areas. Swedish energy and energy research policy is a good case in point. These public programs have always been considered as means of solving problems in other policy areas as well as their defined energy policy goals. For instance, hydroelectric power development would not only limit coal imports but also create domestic employment opportunities, be an important element in regional development policy, and secure industrial expansion. Similarly, there have been multiple motives behind Sweden's nuclear power policy, motives connected with trade policy, industrial policy, labor market policy, and environmental policy. Basically, the same instrumental arguments can be historically identified to justify investments in domestic fuel resources such as peat, shale, and wood.

Three broad classes of objectives have been relevant in the formation of Swedish energy policy. First, there have been objectives reflecting a desire to promote national self-reliance and to avoid negative effects produced by possible disruptions or disturbances on international energy markets. These objectives have translated into efforts to increase the use of domestic energy sources. A second group of objectives has concerned trade and industrial policy. These objectives have also tended to be manifested by demands for a reduction in energy imports which create a strain on the critical Swedish balance of payments issue. Conversely, one rationale for the development of domestic energy technologies has been the desirability of supporting industry, employment, and exports. Finally, a third class of goals is that the energy system is developed according to principles of economic feasibility and market-defined profitability, with a minimum of government intervention. These boundaries are, of course, obscured by the realization that energy is often treated as a "public good."

Naturally, these three classes of objectives and the concomitant public measures have had an effect on "implementation by defining the arena in which the process takes place, the identity and role of the principal actors, the range of permissible tools for action, and, of course, by supplying resources" (Majone and Wildavsky, 1979: 188). But the range of objectives has often been sufficiently disparate to permit an adoption of programs to changing situations and contexts. To a certain extent, significant aspects of policies can survive even substantial changes in their environments through a process which could be termed *policy drift*. A gradual shift in the emphasis of various objectives occurs, some programmatic components are toned down or even discarded, but the program itself is able to survive at least as defined in a step-by-step or incremental fashion, regardless of its efficacy in terms of the original objectives and policies (cf., Ascher, 1978, and his concept of "policy lag"). The outer limits of policy survival can even be stretched still further if policymakers resort to symbolic policymaking (see Gustafsson, 1983; Barrett and Fudge, 1981). One is reminded of Majone's and Wildavsky's (1979a) "implementation as evolution," but, at some point, policies must "lose their legs" and "no longer travel;" they go astray or rather get sidetracked or even abandoned by a changing set of structural constraints and contextual requirements (cf. Knoepfel and Weidner, 1982).

In the case of Swedish energy policy, the outlines of a cyclical pattern can be discerned. During periods of great uncertainty and crisis-like events, the government launched ambitious programs which entailed fairly far-reaching control and intervention in the processes of technology development and innovation (Wittrock, Lindstrom, Zetterberg, 1982). The main objectives emphasized national self-reliance and trade policy. The programs typically involved substantial efforts to stimulate technologies which might tap domestic energy sources. These R&D programs often reflected a long-range research orientation and rested upon assumptions of the future profitability in market terms of technologies under development. The state also assumed the main responsibility in financial support. However, the periods of crisis have tended (almost by definition) to be of fairly short

duration. When the immediate sense of uncertainty waned, the political feasibility of far-reaching state intervention, support, and control similarly subsided; even governmental organizations have only limited attention spans and such interventions seemed no longer necessary. Furthermore, their potential incompatibility with the basic operating mode of private industry was highlighted at precisely the moment when the R&D results reached a stage where their entry into the market was put on the agenda.

In short, then, we find a changing context and set of actors (and their respective emphases) without a concomitantly changing set of implementation strategies. New policy asymmetries have arisen. Rather than reestablish a new set of implementation strategies to match the new situations, more often than not, the old ones are permitted to continue in place--sometimes benignly melting away, occasionally proving counterproductive--with the effect rarely being that which the policymaking bodies had envisioned. Policy drift has effectively replaced vigilance as a response to these systemic asymmetries, even in those cases in which the emerging asymmetries were predictable. Again, this reflects the generally accepted assumption that policy is a stable phenomenon and the policy process a stable one, at least for planning purposes. Scarce wonder then that the congruence between policy expectations and policy effects is rarely realized.

IV. CONCLUDING OBSERVATIONS

Implementation now faces--to use Wildavsky's expression (Wildavsky, 1984: xvii)--a "mid-life crisis." As an intellectual endeavor it is "shaken from its safe cognitive anchorage in prior objectives and future consequences that do or do not measure up to original expectations" (ibid.). In an age when many traditional policy objectives--be they balanced budgets, sustained economic growth, or full employment--appear increasingly difficult to attain and the resources to do so scarcer than for a long time, the policy imperative of implementation analysis is embarrassingly obvious. However, it appears to be an entirely open question whether implementation analysis as a scholarly endeavor will come out its "mid-life crisis" with increased vitality or whether it will become submerged in "an imperialized hodge-

podge" (ibid.) of social and policy studies. The fertile middle-ground claimed by today's more imaginative implementation analysis might well be expanded. However, we might also come to witness how students of implementation processes either come to focus on narrowly defined issues of organizational design or else let policy orientation give way to general and detached social theorizing.

We have argued that perhaps the one "constant" in the policy process is change. Some might claim that the process is cyclical (May and Wildavsky, 1978), which might make it more amenable to policy planning, but such regularity is far from proven. For the present analysis, we should operate under the assumption that alterations in the policy environment are to be expected and, to a large extent, unpredictable, or at least not particularly susceptible to confident foresight. This assumption, as we have stated above, has profound implications for a theory of implementation and serious consequences for even the more modest goals of the implementation analysts. Simply stated, the dynamics inherent in the implementation processes can no longer be neglected, however inconvenient that must be. The recognition and inclusion of this variability, however, will make for a more secure theoretical base. On a more fundamental level this also raises the issue of the prospects of dispositional and evolutionary accounts of policy change and policy innovation (cf. Knoepfel and Weidner, 1982; Majone and Wildavsky, 1979; Sabatier and Hanf, 1985; Wittrock, 1985).

We suggest that there are three initial steps towards a mode of analysis of implementation processes which combines policy relevance with conceptual realism. First, the development of a typology of policies which would differentiate as a function of the time horizon. This would permit planners to select implementation strategies which would explicitly take contextual and temporal variabilities at least partially into account (Williams, 1975). In many cases, these policies or programs would not be of heroic stature but, for this effort, walking is more important than running. Second, Berman's (1980) concept of "adaptive" implementation openly heeds the irregularities among the program recipients and perhaps even encourages them. Berman and McLaughlin (1974) document the viability of such strategies in terms of

a nationally funded Change Agents program for encouraging educational innovation; Robertson (1984) emphasizes how different design features of job training programs fundamentally influenced why these programs suffered severe implementation failures (also see Majone and Wildavsky, 1979). The problem, of course, is to recognize what types of program are suitable to such flexibility, especially in light of compliance requirements. Still, we would urge greater as opposed to lesser latitudes in implementation strategies as they occur on both the theoretical and applied levels of research. Third, given the fact that "more often than not, all other conditions do *not* remain equal," i.e., the *ceteris paribus* clause, upon which so much policy design implicitly rests often does not hold, "social scientists can use their analytic techniques to demonstrate what cannot be expected to happen or what is most unlikely to happen or to warn of unwanted likely consequences" (Rose, 1982: 6). Such uses of analysis are entirely in line with the scholarly commitments of the policy sciences. They also have an immediate relevance for policy design and policy modification. Thus a major contribution to the avoidance of "planning disasters" (Hall, 1980) is constituted by "research that demonstrates the absence of logical or empirical evidence to suggest that a new policy proposal would resolve a problem" (Rose, 1982: 6). Analyses of this type, modest though they may appear, will certainly also help pave the way for the other two steps suggested above, the development of a typology of policies in terms of time horizon and an increased, but carefully selective, reliance on strategies of adaptive implementation.

We do not claim here that the tasks before the implementation researcher are easy. The earlier recapitulation of the problems encountered by numerous scholars give ample testimony to their difficulty and complexity (Hucke and Wollman, 1980). We do, however, make two counterclaims. First, the dual tasks of theoretical and practical realism for implementation is essential if policy analysts and decisionmakers are to close the gap between policy inputs and policy results, i.e., between expectations and effects. This gap, so well illustrated by a host of policy implementation horror stories, might be understandable in retrospect, but in a period characterized by contracting resources and rising expectations, it can scarcely be

excused and permitted to continue. Second, and more optimistically, the policy sciences, with their emphases on problem-oriented, broad contextual approaches, would appear to have important intellectual insights on how this gap might be bridged. Furthermore, we are hopeful that the professional growth of the field alluded to at the beginning of this analysis, if thoughtfully managed, can provide the skilled human resources towards such ends. This paper has posed some initial bricks for that bridge with the explicit trust that others will take up similar constructs and mortar. Although we cannot be certain what the edifice will ultimately resemble, we can rest assured of its utility.

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